THE ROCKEFELLER INSTITUTE
FOR MEDICAL RESEARCH
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Dear Josh,

Cross fire need not necessitate formality, especially when it comes of honest disagreement coupled perhaps with misunderstanding.

Of course we should share any technical ob experimental advances that would be mutually of aid. In time you will probably find yourself bothered by so much of experimental and hypothetical triviality as to almost regret this offer. I have for so long had you for a sounding board and hold your criticism in so high regard that I shall as often as possible avail myself of it. Enough of this and to business.

I haven't had the gal-duction story complete. When I left it was still very hypothetical and since I've but heard dribs and drabs. From Stocker, from your last letter and from the recombinational association of gal and lambda (which does seem to be more than coincidence in relation to gal-duction) I inferred that the phage played a more specific role. My concern with this notion comes from the fact that the phage workers are now convinced that prophage sits on the chromosome and seem to be believe that transduction provides evidence for this, which I for one do not see. However, keeping this in mind I've sought to rationalize the difference between gal and trans-duction in those terms, specifically with reference to the method of lysate production. You did not answer this point or is it too far-fetched? What are your views on this matter?

I've just about completed the first set of interference experiments. They are more laborious than difficult. Of twelve clones transduced with mutant phage, eight had the parent and four the mutant. I could not be certain of the purity of the phage clones as a third , not too different phage crept in , as SW-351 (to be transduced) was self-lytic. The experiment has been set up again with a fresh batch of cells.

The new high titer phage preparations have entirely different U.V. adsorption than those I have had. There is a very definate rise at 2600 with a soft shoulder. The air in these parts seems to be permeated with DNA. Seriously though I'm at a loss to explain this as the titers read were comparable . I shall prepare some via the old method for comparison.

Was much interested in yuor progeny tests of SW-543 transinductions. Spicer and I had just discussed this most critical point and I had just looked into the question of which phages could be used. Your results however surprised me. Arent all non-motile cells of genotype A^{x} B-? The difference here being that the B- here is allele specific suppressor while in the other strains it is non-specific. The linked transinductions in Hemophilus both separate and stay together in progeny test.

I've not completely forgotten about the U.V. activation , but still have not found much direct use to put it to other than to answer such questions as are transdinduced cells (transduced with U.V. inactivated phage to which they are susceptible) refractory to his phage. In other words can inactivated phage induce a sort of masked lysogenicity.

I've been assaying transduction with different LT-7 singles using PLT-22/2 to which they are susceptible. I shall compare this with PLT-22/7. The phage to FA ratio has been 10^5 with SW-191. I agree that plaque formation need not result in the loss of a clone and this is evidenced by the fact that SW-191 gives more transductions than SW-188 as it has a greater residual growth on minimal medium. These assays are of course done where the number of transductions should be independent of bacterial number and even with residual growth no spontaneous reversions are expected. I asked for the other sets of singles to continue these comparisons. (While I remember try PLT-22 with S.typhi #57 for FA.)

We haven't as yet looked into why SW-435 is avirulrnt as mice not being bottles of culture media are not always available. However the culture is streptomycin resistant and some recent titrations of /S virulents have shown them to be avirulent (at least four decades different in challenge size tolerated). This of course will merit much further attention.

Plough's recent paper has stirred things up a bit and I've been questioned for an explanation which I can not give. Again his multiple markers are suspect especially in view of their separation in "transduction" i.e. isoleucine-valine, but I must admit I don't know.

Bernie was over to lunch the other day and we discussed the nomenclature problem. We agreed that regardless of the validity or lack thereof of Horowitz's arguments it is too late for much to be done about it. He is going to ask him to withdraw the note and let sleeping dogs lie.

You were right about my getting used to giving my little talk as I am about to embark on a speaking tour not much different from those our presidential candidates are undergoing although I hope less controversial. I have thus far six engagements between now and December and of course I can't say no.

Best regards to all

Sincerely,

Norton

Norton